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XXII. *New Experiments in Electricity: In a Letter from Mr. Ebenezer Kinnerley, to Benjamin Franklin, LL. D. F. R. S.*

Read Nov. 18, 1762, March 24, and April 14, 1763.

S I R,

Philadelphia, Mar. 12, 1761.

HAVING lately made the following experiments, I very chearfully communicate them, in hopes of giving you some degree of pleasure, and exciting you to further explore your favourite, but not quite exhausted, subject, ELECTRICITY.

E X P. I.

I placed myself on an electric stand, and, being well electrified, threw my hat to an unelectrified person, at a considerable distance, on another stand, and found, that the hat carried some of the electricity with it; for, upon going immediately to the person, who received it, and holding a flaxen thread near him, I perceived he was electrified sufficiently to attract the thread.

E X P. II.

I then suspended, by silk, a broad plate of metal, and electrified some boiling water under it, at about four feet distance, expecting that the vapour, which ascended plentifully to the plate, would, upon the principle of the foregoing experiment, carry up some of the electricity with it; but was at length fully convinced, by several repeated trials, that it left all its
share

share thereof behind. This I know not how to account for ; but does it not seem to corroborate your hypothesis, that the vapors, of which the clouds are formed, leave their share of electricity behind in the common stock, and ascend in a negative state ?

E X P. III.

I put boiling water into a coated Florence flask, and found that the heat so enlarged the pores of the glass, that it could not be charged. The electricity passed thro' as readily, to all appearance, as thro' metal ; the charge of a three-pint bottle went freely thro' without injuring the flask in the least. When it became almost cold, I could charge it as usual. Would not this experiment convince the Abbé Nollet of his egregious mistake ? For, while the electricity went fairly thro' the glass, as he contends it always does, the glass could not be charged at all.

E X P. IV.

I took a slender piece of cedar, about eighteen inches long, fixed a brass cap in the middle, thrust a pin, horizontally and at right angles, thro' each end, (the points in contrary directions) and hung it, nicely balanced like the needle of a compass, on a pin, about six inches long, fixed in the center of an electric stand. Then electrifying the stand, I had the pleasure of seeing what I expected ; the wooden needle turned round, carrying the pins with their heads foremost. I then electrified the stand negatively, expecting the needle to turn the contrary way ; but was extremely disappointed, for it went still the same way as before.

When the stand was electrified positively, I suppose, that the natural quantity of electricity in the air being increased on one side, by what issued from the points, the needle was attracted by the lesser quantity on the other side. When electrified negatively, I suppose, that the natural quantity of electricity in the air was diminished near the points; in consequence whereof, the equilibrium being destroyed, the needle was attracted by the greater quantity on the opposite side.

The doctrine of repulsion in electrified bodies, I begin to be somewhat doubtful of. I think all the phenomena, on which it is founded, may be well enough accounted for without it. Will not cork balls, electrified negatively, separate as far as when electrified positively? And may not their separation, in both cases, be accounted for upon the same principle; namely, the mutual attraction of the natural quantity in the air, and that which is denser, or rarer in the cork ball? It being one of the established laws of this fluid, that quantities of different densities shall mutually attract each other, in order to restore the equilibrium.

I can see no reason to conclude, that the air has not its share of the common stock of electricity as well as glass, and, perhaps, all other electrics per se. For tho' the air will admit bodies to be electrified in it either positively or negatively, and will not readily carry off the redundancy in the one case, or supply the deficiency in the other;

E X P. V.

Yet let a person in the negative state, out of doors in the dark, when the air is dry, hold, with his arm extended, a long sharp needle, pointing upwards; and he will soon be convinced, that electricity may be drawn out of the air; not very plentifully, for, being a bad conductor, it seems loth to part with it; but yet some will evidently be collected. The air near the person's body, having less than its natural quantity, will have none to spare; but, his arm being extended as above, some will be collected from the remoter air, and will appear luminous as it converges to the point of the needle.

Let a person electrified negatively present the point of a needle, horizontally, to a cork ball suspended by silk, and the ball will be attracted towards the point, till it has parted with so much of its natural quantity of electricity as to be in the negative state, in the same degree with the person who holds the needle: then it will recede from the point; being, as I suppose, attracted the contrary way by the electricity of greater density in the air behind it. But, as this opinion seems to deviate from electrical orthodoxy, I should be glad to see these phænomena better accounted for by your superior and more penetrating genius.

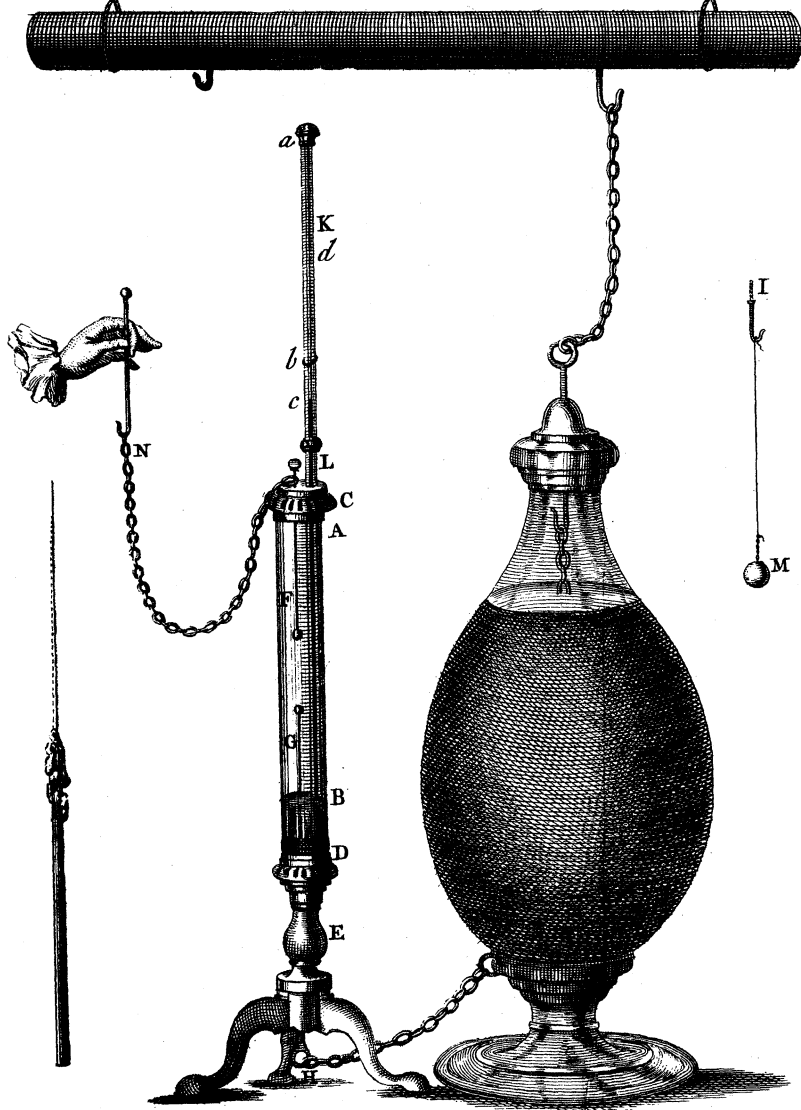
Whether the electricity in the air, in clear dry weather, be of the same density at the height of two or three hundred yards, as near the surface of the earth, may be satisfactorily determined by your old experiment of the kite.

E X P.

E X P. VI.

The twine should have, through-out, a very small wire in it; and the ends of the wire, where the several lengths are united, ought to be tied down with a waxed thread to prevent their acting in the manner of points. I have tried the experiment twice, when the air was as dry as we ever have it, and so clear that not a cloud could be seen; and found the twine each time, in a small degree electrified positively. The kite had three metalline points fixed to it; one on the top, and one on each side. That the twine was electrified, appeared by the separating of two small cork balls suspended on the twine by fine flaxen threads, just above where the silk was tied to it, and sheltered from the wind. That the twine was electrified positively, was proved by applying to it the wire of a charged bottle; which caused the balls to separate further, without first coming nearer together. This experiment shewed, that the electricity in the air, at those times, was denser above than below. But that cannot be always the case; for you know we have frequently found the thunder clouds in the negative state, attracting electricity from the earth. Which state it is probable they are always in when first formed, and till they have received a sufficient supply. How they come afterwards, towards the latter end of the gust, to be in the positive state, which is sometimes the case, is a subject for further enquiry.

After the above experiments with the wooden needle, I formed a cross of two pieces of wood, of
equal



equal length, intersecting each other at right angles in the middle; hung it, horizontally, on a central pin, and set a light horse, with his rider, upon each extremity; whereupon, the whole being nicely balanced, and each courser urged on by an electrified point, instead of a pair of spurs, I was entertained with an electrical horse-race.

I have contrived an electrical air thermometer, and made several experiments with it, that have afforded me much satisfaction and pleasure. It is extremely sensible of any alteration in the state of the included air, and fully determines that controverted point, whether there be any heat in the electric fire. By the Plate [TAB. IV.] and the following description, you will readily apprehend the construction of it.

A. B. is a glass tube about eleven inches long, and one inch diameter in the bore. It has a brass feril cemented on each end, with a top and bottom part, C and D to be screwed on, air-tight, and taken off at pleasure. In the center of the bottom part D, is a male screw, which goes into a brass nut in the mahogany pedestal E. The wires F and G are for the electric fire to pass through, darting from one to the other. The wire G extends through the pedestal to H; and may be raised or lowered by means of a male screw on it. The wire F may be taken out, and the hook I be screwed into the place of it. K is a glass tube with a small bore, open at both ends, cemented in the brass tube L, which screws into the top part C. The lower end of the tube K is immersed in water, coloured with cocheneal, at the bottom of the tube A B. (I used at first coloured spirits of wine; but, in one of the experiments I made, it took fire.)

On the top of the tube K is cemented, for ornament, a brass feril, with a head screwed on it, which has a small air hole thro' its side at *a*. The wire *b* is a small round spring, that embraces the tube K so as to stay wherever it is placed. The weight M is to keep strait whatever may be suspended in the tube A B. on the hook I. Air must be blown thro' the tube K into the tube A B, 'till enough is intruded to raise, by its elastic force, a column of the coloured water, in the tube K up to *c*, or thereabouts; and then, the gage wire *b* being slipt down to the top of the column, the thermometer is ready for use.

E X P. VII.

I set the thermometer on an electric stand, with the chain N fixed to the prime conductor, and kept it well electrified a considerable time; but this produced no sensible effect. Which shews, that the electric fire, when in a state of rest, has no more heat than the air and other matter wherein it resides.

E X P. VIII.

When the wires F and G are in contact, a large charge of electricity sent thro' them, even that of my case of five and thirty bottles, containing above thirty square feet of coated glass, will produce no rarefaction of the air included in the tube A B. Which shews, that the wires are not heated by the fires passing thro' them.

E X P. IX.

When the wires are about two inches apart, the charge of a three pint bottle, darting from one to the
the

the other, rarefies the air very evidently. Which shews, I think, that the electric fire must produce heat in itself, as well as in the air, by its rapid motion.

The charge of one of my glass jars, which will contain about five gallons and a half, wine measure, darting from wire to wire, will, by the disturbance it gives the air in the explosion repelling it in all directions, raise the column in the tube K up to *d*, or thereabouts; and the charge of the above-mentioned case of bottles will raise it to the top of the tube. Upon the air's coalescing, the column, by its gravity, instantly subsides till it is in equilibrio with the rarefied air; it then gradually descends, as the air cools, and settles where it stood before. By carefully observing at what height above the gage-wire *b* the descending column first stops, the degree of rarefaction is discovered; which, in great explosions, is very considerable.

E X P. X.

I hung in the thermometer, upon the hook I, successively, a strip of wet writing paper, a wet flaxen and woolen thread, a blade of green grass, a filament of green wood, a fine silver thread, a very small brass wire, and a strip of gilt paper; and found that the charge of the glass jar, passing thro' each of these, especially the last, produced heat enough to rarify the air very perceptibly. The charge of the case of bottles sent thro' the brass wire consumed great part of it into smoke. The thermometer appeared quite opaque with it.

E X P. XI.

I then suspended, out of the thermometer, a piece of brass wire, not quite so small as the former, about twenty four inches long, with a pound weight at the lower end; and, by sending the charge of the case of bottles thro' it, discovered a new method of wire-drawing. The wire was red hot, the whole length well annealed, and above an inch longer than before. A second charge melted it; it parted near the middle, and measured, when the ends were put together, four inches longer than at first. This experiment I remember you proposed to me, as worth trying, before you left Philadelphia; in order to find, whether the electricity, in passing thro' the wire, would so relax the cohesion of its constituent particles, as that the weight might produce a separation; but neither of us had the least suspicion, that any heat would be produced.

E X P. XII.

That I might have no doubt of the wire's being *hot* as well as red, I repeated the experiment on another piece of the same wire, encompassed with a goose-quill filled with loose grains of gun-powder; which took fire as readily, as if it had been touched with a read hot poker. Also tinder, tied to another piece of the wire, kindled by it. I tried a wire about twice as big, but could produce no such effects with that.

Hence it appears, that the electric fire, tho' it has no sensible heat when in a state of rest, will, by its
violent

violent motion, and the resistance it meets with, produce heat in other bodies, when passing thro' them, provided they be small enough. A large quantity will pass thro' a large wire without producing any sensible heat; when the same quantity passing thro' a very small one, being there confined to a narrower passage, the particles crowding closer together, and meeting with greater resistance, will make it red hot, and even melt it.

Hence lightning does not melt metal by a cold fusion, as we formerly supposed. But when it passes thro' the blade of a sword, if the quantity be not very great, it may heat the point so as to melt it, while the broadest and thickest part may not be sensibly warmer than before.

And when trees or houses are set on fire by the dreadful quantity, which a cloud, or the earth sometimes discharges, must not the heat, by which the wood is first kindled, be generated by the lightning's violent motion thro' the resisting combustible matter?

If lightning, by its rapid motion, produces heat in itself as well as in other bodies, (and that it does, I think, is evident from some of the foregoing experiments made with the thermometer) then its sometimes singeing the hair of animals killed by it may easily be accounted for. And the reason of its not always doing so may, perhaps, be this: the quantity, tho' sufficient to kill a large animal, may, sometimes, not be great enough, or not have met with resistance enough, to become by its motion burning hot.

We find, that dwelling houses, struck with lightning, are seldom set on fire by it; but when it passes thro'

thro' barns with hay or straw in them, or store-houses containing large quantities of hemp, or such like matter, they seldom, if ever, escape a conflagration. Which may, perhaps, be owing to such combustibles being apt to kindle with less degree of heat than is necessary to kindle wood.

We had four houses in this city, and a vessel at one of the wharfs, struck, and damaged, by lightning last summer. One of the houses was struck twice in the same storm. But I have the pleasure to inform you, that your method of preventing such terrible disasters, has, by a fact, which had like to have escaped our knowledge, given a very convincing proof of its great utility, and is now in higher repute with us than ever.

Hearing, a few days ago, that Mr. William West, merchant in this city, suspected, that the lightning, in one of the thunder-storms last summer, had passed through the iron conductor, which he had provided for the security of his house, I waited on him, to enquire what ground he might have for such suspicion. Mr. West informed me, that his family and neighbours were all stunned with a very terrible explosion, and that the flash and crack were seen and heard at the same instant. Whence he concluded, that the lightning must have been very near; and, as no house in the neighbourhood had suffered by it, that it must have passed through his conductor. Mr. White, his clerk, told me, that he was sitting at the time by a window, about two feet from the conductor, leaning against the brick wall, with which it was in contact; and that he felt a smart sensation, like an electric shock, in that part of his body, which touched the wall.

Mr.

Mr. West further informed me, that a person of undoubted veracity assured him, that, being in the door of an opposite house on the other side of Water-Street (which you know is but narrow) he saw the lightning diffused over the pavement, which was then very wet with rain, to the distance of two or three yards from the foot of the conductor. And that another person of very good credit told him, that he, being a few doors off, on the other side of the street, saw the lightning above, darting in such direction, that it appeared to him to be directly over that pointed rod.

Upon receiving this information, and being desirous of further satisfaction, there being no traces of the lightning to be discovered in the conductor, as far as we could examine it below, I proposed to Mr. West our going to the top of the house to examine the pointed rod; assuring him, that, if the lightning had passed thro' it, the point must have been melted; and, to our great satisfaction, we found it so. This iron rod extended in height about nine feet and a half above a stack of chimnies, to which it was fixed; (but I suppose, three or four feet would have been sufficient). It was somewhat more than half an inch diameter, in the thickest part, and tapering to the upper end. The conductor, from the lower end of it to the earth, consisted of square iron nail rods, not much above a quarter of an inch thick, connected together by interlinking joints. It extended down the cedar roof to the eaves, and from thence down the wall of the house, four story and a half, to the pavement in Water-Street; being fastened to the wall, in several places, by small iron hooks. The lower end was
fixed

fixed to a ring in the top of an iron stake, that was driven about four or five feet into the ground. The above mentioned iron rod had a hole in the top of it, about two inches deep, wherein was inserted a brass wire, about two lines thick, and, when first put there, about ten inches long, terminating in a very acute point; but now its whole length was no more than seven inches and a half, and the top very blunt. Some of the metal appears to be missing; the slenderest part of the wire being, as I suspect, consumed into smoke. But some of it, where the wire was a little thicker, being only melted by the lightning, sunk down, while in a fluid state, and formed a rough irregular cap, lower on one side than the other, round the upper end of what remained, and became intimately united therewith.

This was all the damage, that Mr. West sustained by a terrible stroke of lightning. A most convincing proof of the great utility of this method of preventing its dreadful effects. Surely it will now be thought as expedient to provide conductors for the lightning as for the rain.

Mr. West was so good as to make me a present of the melted wire; which I keep as a great curiosity, and long for the pleasure of shewing it to you. In the mean time, I beg your acceptance of the best representation I can give of it; which you will find by the side of the thermometer, drawn in its full dimensions as it now appears. The dotted lines above are intended to shew the form of the wire before the lightning melted it.

And now, Sir, I most heartily congratulate you on the pleasure you must have in finding your great and
well

well-grounded expectations so far fulfilled. May this method of security from the destructive violence of one of the most awful powers of nature meet with such further success, as to induce every good and grateful heart to bless God for the important discovery. May the benefit thereof be diffused over the whole globe. May it extend to the latest posterity of mankind; and make the name of FRANKLIN, like that of NEWTON, *immortal*.

I am, Sir, with sincere respect,
your most obedient, and
most humble servant,
Ebenezer Kinnerfley.

XXIII. *Observations in Electricity and on a Thunder-storm: In a Letter from Mr. Torbern Bergman, to Mr. Benjamin Wilson, F. R. S. Acad. Reg. Upsal. Soc.*

Amplissime atque Celeberrime Domine,

Read April 14, 1763. **I**N epistolis recentissimis, quibus me honorasti, experimenta domini Delaval circa electricitatem crystalli Islandicæ commemoras. Pluries hæcce tentamina iteravi, sed constanter eventu prorsus contrario. Scilicet in hunc finem varia hujus crystalli frustra frigori 12 graduum exposui,
VOL. LIII. P in